

steady and without the slightest shake. It is this system of centring that constitutes the patent by which Messrs. Batchelor's drawings are protected. The pivots are made entirely of card and paper cut in a most ingenious manner, by which both freedom and steadiness are insured. Nor are the centres the only parts of these drawings interesting for their ingenuity; the contrivance for holding down the sliding parts is equally good. It consists of a band of thin paper passing over the sliding part, and printed exactly like the part it covers, so that it is invisible except on close examination.

The first of these drawings which is before us is a diagram in illustration of the action of the "trunk engine," the characteristic feature of which consists in making the piston-rod hollow and of sufficient internal diameter to allow the connecting-rod to be attached at one end direct to the piston, and to oscillate within the trunk, the other end embracing the crank-pin. By this means the crank shaft can be brought nearer to the cylinder, considerable space thereby being saved, and the alternative system by which direct connection between the piston and crank is effected, viz., the oscillating cylinder, is avoided, with its more complicated valve gear and expensive construction.

The double trunk system represented in the drawing was the invention of the late Mr. John Matthew, who for many years was a partner in the eminent firm of Messrs. John Penn and Sons, and it is the system upon which, almost without exception, the large screw engines of Messrs. Penn are constructed, with which so many of the ships in her Majesty's navy are fitted.

There is nothing to be desired in the execution of the diagram before us, of which the name of Messrs. Maclure and Macdonald is a sufficient guarantee. It is lithographed in white upon a blue ground, and all the parts come out with singular distinctness. We could have wished that, in the choice of an example for illustration, a more modern design of engine had been selected. The eccentric rod, with its lattice bracing, is that employed in the old beam engines, and a trunk engine made to the drawing before us could hardly work, for the crank pin is evidently inserted into one of the spokes of the fly-wheel, and unless projecting to an impossible extent, the trunk could not clear the wheel; this could very easily have been remedied by showing the "throw" of a crank behind the connecting-rod, which would have aided rather than detracted from the clearness of the diagram.

While thus criticising the particular design of engine selected for representation, we can only express admiration of this most ingenious system of illustrating mechanical motions and the action of machines. For educational purposes it will be of the highest value, and there are many of the examples in Reuleaux's masterly work upon the "Kinematics of Machinery,"¹ so ably translated by Prof. Kennedy, to which it might with great advantage be applied.

We feel sure that Messrs. Batchelor's drawings will be a great boon to inventors for explaining their inventions to others; and as supplementary to scientific evidence in disputed patent cases and other litigation, they will be found of value.

C. W. C.

¹ "Theoretische Kinematik."

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Oxygen in the Sun

FROM the time of the discovery by Prof. Draper of the presence of oxygen in the sun down to the present moment I have devoted most of my leisure time to the consideration of the question as to why the oxygen lines should appear *bright* while the metallic lines should appear dark in the solar spectrum. I was led into this inquiry under the firm belief that the new fact made known by Dr. Draper might lead to a modification of existing views of the sun's atmosphere, and it was consequently with the greatest pleasure that I read in last week's NATURE a communication from Dr. Schuster on this subject.

The views which I have arrived at being in my opinion hardly matured enough for publication, I reserve further statement at present, but will so far anticipate as to say that the explanation which I am disposed to maintain necessitates the assumption that oxygen possesses two different spectra—a low temperature band-spectrum and the well-known line-spectrum of high temperatures. This assumption I thought warranted by the behaviour of other non-metals as made known by the researches of many spectroscopists, but more particularly by those of Salet and Lockyer, and endorsed by the low temperature absorption spectra of the metals discovered by Roscoe and Schuster, Lockyer and Roberts. The recent research of Dr. Schuster, however, has now placed this assumption in the position of a fact, and all who have followed recent spectroscopic advancement will recognise the value and importance of this last discovery.

As Dr. Schuster's explanation of the brightness of the oxygen lines differs fundamentally from that which I am inclined to hold, and as he considers this view warranted by the result of his investigation, I will beg permission to make a few brief remarks upon the chief points of difference between us, being convinced that their discussion cannot fail to elicit opinions of interest to all concerned in the progress of solar physics. This ventilation of opinion is the more necessary as views very similar to, if not identical with, those of Dr. Schuster's had occurred to me and had been abandoned for reasons which I will now explain.

According to Dr. Schuster, "the temperature of the sun, at some point intermediate between the photosphere and the reversing layer" is the same as that at which the spectrum of oxygen changes; that is to say, if I rightly interpret these words, *above the photosphere* the temperature is such that we get the line spectrum of oxygen and *above the reversing layer* the temperature is such that we get the band ("compound line") spectrum. This state of affairs would doubtless account for the reversal of the "compound line" spectrum which Dr. Schuster has now shown to be present in the solar spectrum, but I fail to see at present how it is to be reconciled with the *bright* line oxygen spectrum. Let us consider the conditions more closely. All observers agree in placing the reversing layer at the base of the chromosphere—the present hypothesis necessitates a space between the photosphere and the reversing layer—i.e., a space sufficiently extended to contain the incandescent oxygen giving the line spectrum. Neglecting for the present the antagonism between these views, let us assume that such a space exists, and for the sake of simplicity let us also neglect the other elements which may be present. Now it cannot be assumed that the supposed zone is *higher* in temperature than the photosphere—it *might be* of the same temperature, but, being above the photosphere it would more probably be at a *lower* temperature. Let us, however, make the assumption most favourable to Dr. Schuster's view, viz., that the hypothetical zone is of the same temperature as the photosphere. Then we have a zone of oxygen exterior to the photosphere and of the same temperature as this last region, and above the oxygen the cooler reversing layer. Thus the light of the photosphere passes unchanged through the oxygen zone, and we should see no dark lines corresponding to the line-spectrum of this gas.—As a matter of fact, however, the oxygen lines are *bright*—hence it must be at a higher temperature than the photosphere, or we must be looking through an enormous stratum of

it, a stratum thick enough for the radiation of the gas to overpower the fierce glare of the photosphere behind it, and both these views have been shown to be untenable.

December 21

R. MELDOLA

Oxygen in Sea-water

AT p. 267 of the second volume of the "Voyage of the *Challenger*," Sir Wyville Thomson writes:—

"Mr. Buchanan drew the conclusion in explanation of the small amount of oxygen at depths of 300 fathoms and upwards, 'that animal life must be particularly abundant and active at this depth, or at least more abundant than at greater depths.' In other words, that a permanent condition, probably of all conditions the most unfavourable to animal life, is produced and maintained by its excess."

"This is entirely contrary to experience."

The words in inverted commas are part of a sentence in a short report in *NATURE* (vol. xvi. p. 255), of a paper which I read before the Royal Society of Edinburgh, on the results of the analysis of so many of the samples of air extracted during the cruise, from sea-water of different sources, as I was able to accomplish before my connection with the work of the expedition ceased. I will not encroach on your valuable space by anticipating the discussion of the bearing of my observations and those of others on the question of the greater or less abundance of animal life at different depths in the sea; but as the above quotation, from its fragmentary character, is somewhat misleading, both as to the nature of the belief which I expressed and my grounds for holding it, I must ask you to give place to the concluding sentences of the above report:—

"It is evident from these figures¹ that between 200 and 400 fathoms there is a great consumption of oxygen going on, and, as it is difficult to conceive its being consumed otherwise than by living creatures, the conclusion is forced on us that animal life must be particularly abundant and active at this depth, or, at least, more abundant than at greater depths; for at less depths there is more opportunity for renewal of the oxygen by reason both of the greater proximity to the surface and of the existence of vegetable life. This conclusion was borne out by the numerous experiments made by Mr. Murray with the tow-net at intermediate depths, which went to prove the existence of abundance of animal life down to 400 fathoms, vegetable life never extending to much below 100 fathoms. Below 400 fathoms life is sparingly met with."

It will be seen that the only independent experience which exists, namely, Mr. Murray's observations with the tow-net at different depths, is in favour of the conclusion at which I arrived.

J. Y. BUCHANAN

10, Moray Place, Edinburgh, December 13

On some Peculiar Points in the Insect-Fauna of Chili

FOR some years past I have been particularly interested in some points in the entomology of Chili and the extreme southern portion of South America, which, although known to most entomologists who have made special groups their study, have never yet been, so far as I know, even more than casually alluded to in works on geographical distribution, and are ignored in the principal ones. I allude to the occurrence in that part of the world of well-marked palaearctic or nearctic forms not found otherwise in America south of Mexico, and utterly unknown in the southern hemisphere in the Old World.

I have collected a not inconsiderable amount of data concerning this subject, and have the intention of addressing a circular to zoologists and also to botanists, asking for further information.

I will here allude to such familiar genera as *Carabus* amongst beetles and *Argynnis* and *Colias* amongst butterflies. *Carabus* is very abundant in species in the palaearctic region, poor in the nearctic, and reappears (for the whole world) only in Chili. The distribution of *Argynnis* and *Colias* is similar, only that they are about equally abundant in the two northern regions, and of *Colias* it appears probable that a single species occurs in Peru, but this exception only proves the rule.

In the *Trichoptera*, or Caddis-flies, a group of insects in which I am especially interested, there is even a still more striking case. The typical family, *Limnophilidae*, comprising those insects the larvæ of which manufacture the cases of twigs and straws, so

¹ A table of the mean amounts of oxygen in a hundred parts of oxygen and nitrogen contained in waters from different depths

abundant in our ponds and ditches, and which is so rich in species in northern regions, is not, with the exception stated below, known south of Mexico in the New World nor south of the Himalayas in the Old; but I have several species from Chili, Araucania, and the Falkland Isles.

I could already multiply parallel instances, but have said enough to prove my case.

Confessedly I have, at present, only crude theoretical notions on the causes of this anomalous distribution. It might be said that these insects are the remains of a former Antarctic glacial epoch. But if this be so, then we must presuppose the existence of former Arctic and Antarctic faunas similar in details; all other evidence tends, I think, to disprove this. It may truly be said that, owing to the non-existence of large tracts of land towards the south pole at all comparable with those that exist towards the north, we are not in a position to acquire sufficient data, yet we have the continent of Australia and the large islands of New Zealand extending somewhat far south, and they furnish us with no indication whatever of forms parallel with those found in Chili.

It has occurred to me as just possible, that at the conclusion of the northern glacial epoch a few stragglers, instead of wending their way northward, mistook the points of the compass and went southward. But there remains this great difficulty, viz., that, with one possible exception, there are no indications of these forms on the northern portions of the Andes of South America.

I call attention to this subject as one deserving far more consideration than it has hitherto received, and with the idea that, by ventilating it in *NATURE*, I may receive additional information on a point that greatly interests me.

39, Limes Grove, Lewisham

R. MCLACHLAN

Arctic Auroræ

It will probably interest some of your readers to know that in reply to a communication lately addressed by me to the Admiralty I am informed that Captain Sir George Nares reports that although the auroral glow was observed on several occasions between October 25, 1875, and February 26, 1876, true auroræ were seldom observed, and the displays were so faint and lasted so short a time that the spectroscopic results were not considered worthy of a special report. Although the citron line was seen occasionally, on only two occasions was it well defined, and then for so short a time that no measure could be obtained. A report is preparing with a view to compare the auroral displays with magnetic disturbance, meteorological changes, and other phenomena which will include the few spectroscopic observations obtained.

J. RAND CAPRON

Guilddown, December 24

Insects and Artificial Flowers

IN a late number of *NATURE* a short account is given of some experiments recently made by Prof. J. Plateau, of Ghent, as to insects being deceived by artificial flowers. The nature of these experiments is not given, but the result would appear to have been of a somewhat negative character. In connection with the subject the following incident will not, I think, be considered uninteresting. I was coming by one of the lake steamers from Como to Menaggio, in September, 1875, and saw a humming-bird hawk moth, *Macroglossa stellatarum*, fly to some bright-coloured flowers on a lady's hat on deck, and hang, poised over them for a short time, and then fly away. During the process it made one of those short familiar darts off, for a moment, and then returned, after the manner of the moth when disturbed, and it remained long enough to convince me that it had tested the flowers and found them wanting. Another incident comes across my mind while writing this, which, though it does not exactly bear upon the point, yet is of a somewhat kindred nature. I was crossing from Harwich to Antwerp in August of the same year, and as the weather was fine, and the boat crowded, I remained on deck all night. About 4 o'clock in the morning I saw what appeared to be a bird or a bat flying rapidly about the rigging. As I was watching it the funnel of the steamer poured forth a thick column of black smoke, owing to the fresh coaling it had just received. Off went the creature as soon as it perceived the change, or, at all events, as soon as the change took place, and flew for some time in and about the smoke, now darting through it, close to the funnel mouth, and then letting itself be borne along with it, for some distance, as if in sport, looking very strange and weirdlike in the process.